

THURSDAY, NOVEMBER 30, 1876

## FERRIER ON THE BRAIN

*The Functions of the Brain.* By David Ferrier, M.D., F.R.S. With numerous Illustrations. (London: Smith, Elder, and Co., 1876.)

## II.

SINCE it is certain that the movement of a limb may be occasioned by an idea, an emotion, or a sensation, and also by the reflex of an external stimulation; since, moreover, it is certain that such a movement may be arrested by an idea or emotion; and since there is good ground for the hypothesis that the cerebral hemispheres are, if not the sole agents, at any rate indispensable accessories in the production of ideas and emotions, we have every right to conclude that the hemispheres play a part in the normal production of many movements; and that ideas and cerebral processes are the subjective and objective aspects of one and the same effect. But experiment proves that many, if not all, these movements can be executed in the absence of the hemispheres, therefore the hemispheres are not *indispensable* but *accessory* factors. This leads to the question, What part do they play? And this again to the questions, Are all cerebral processes or only some of them, ideas emotions, and sensations, the others being simply molecular movements which propagate their excitation to centres of muscular innervation?

I understand the Hitzig-Ferrier view to be this: One set of cerebral processes, having their centres or terminal stations in a limited region of the cortex, are sensational, emotional, and ideal; another set, having also all their centres or terminal stations in a limited region of the cortex, are motor. I am not quite sure that this representation is exact, for in both writers there is absence of explicit definition. But this much at least may be taken as exact, that they profess to have discovered limited areas of sensory and motor stimulation, and within these areas limited spots for particular sensations and particular movements. The interpretation must, of course, rest on a basis of fact, yet the facts observed may be accepted without forcing acceptance of the interpretation. There is general agreement on the facts with great want of agreement as to the conclusions. I have already suggested that the discoveries of Hitzig and Ferrier are of great importance; but only as finger-posts for anatomists seeking the pathways of stimulation, not as inductive stations for deductive inferences. All we can say at present is that electrical stimulation of certain spots is followed by certain movements; but *how* the stimulation reaches the motor nerves is as dark as before.

Thus far, therefore, the part played by the cerebral process is only recognisable as an incitation; it certainly does not effect the movements, it only incites the motor organs. And *thus far* it is on a level with *peripheral* incitations, such as the incitation of laughter by tickling the sole of the foot; or of vomiting by tickling the fauces. Laughter is a function of a complex apparatus, and this apparatus may be stimulated in very different ways from very different starting-points—an idea, a sight, or a touch. Vomiting, again, follows from a blow on the head, acidity in the stomach, a disgusting sight, a smell, or a tickling

of the throat. No one considers the sole of the foot and the fauces to be centres or terminal stations for the functions of laughter and vomiting. Why, then, when we see movements of the limbs, the eyes, or the tail following stimulation of the cerebral cortex, are we to conclude these movements to have their centres in the cortex? The foot may be removed or the sole rendered insensible, yet still laughter will be stimulated by ideas, sights, &c., as before. In like manner the spot of the cerebral cortex may be removed or destroyed, yet still the limbs will move as before. Nay, not only the cortical spots, but the whole hemisphere may be removed, and still the limbs will move as before.

For anatomical purposes we make a wide distinction between grey matter and white, and a still wider distinction between central and peripheral nerve-substance. Physiologically such distinctions are, I conceive, erroneous, the whole nervous system being one. But the distinction between a centre—*i.e.*, a place *to* which stimulations are carried, and *from* which motor impulses issue—and a peripheral region where stimulations begin or end—is both a physiological and an anatomical division usefully maintained. According to this definition of a centre, we may doubt whether the cortex of the cerebrum has any claim to be called a centre, or group of centres, *whether, in fact, it is not a peripheral region*, the processes of stimulation in it being of the same order as the processes of stimulation in the skin, or mucous membranes, *i.e.*, simply those of peripheral incitation.

This is the paradox to which allusion was made at the close of my former article. As it would occupy too much space for development here, and as I have worked it out in a volume now at press, I merely suggest it for speculative readers, and pass on to Dr. Ferrier's book.

First as to his facts. It has been urged against his localisations that he has employed a too powerful current. On this subject I have no right to an opinion, but incline to accept his reply as satisfactory; though one must not overlook the fact remarked by Carville and Duret, that very different movements may be occasioned by the stimulation of one and the same spot according to intensity of the current—a fact analogous to what is observed in stimulations of the skin. It has been urged by Hitzig, and also by Braun (Eckhard's "Beiträge," vii., 133, 137), that in Dr. Ferrier's experiments the same movements follow stimulation of different spots, even when these spots are situated in the different regions recognised as excitable and non-excitable. The objection is not only triumphantly answered by Dr. Ferrier, but is answered by the introduction of an idea which is of great significance:—"The mere fact," he says, "that movements result from stimulation of a given part of the hemisphere does not necessarily imply that the same is a motor centre in the proper sense of the term. It will afterwards be shown that the movements which result from stimulation of the regions in question are expressive of sensation, and that the character of the movements furnishes an important index to the nature of the sensation" (p. 147; compare also p. 163).

As an answer to his critics this seems conclusive. But does it not throw a serious difficulty in the way of his hypothesis? If, as he luminously suggests elsewhere,

"the sensations accompanying muscular action being repeated as often as the muscular action itself, the organic nexus between the motor and tactile centres becomes so welded, that this sensori-motor cohesion enters like a compound chemical radical as a simple factor into every association which motor centres can form with other motor centres and with sensory centres in general" (p. 268); then surely this, while it satisfactorily accounts for the motions following excitation of sensory regions, leaves unexplained the facts of other such excitations *not* being followed by motions (comp. p. 231), and raises the question whether all motions are not due to sensory excitation? On the first point let us ask why the optic thalami—said by him to be sensory centres—do not respond by motor manifestations when stimulated? He regards this "as sufficient of itself to dispose at once of the views of those who would attribute motor functions to these ganglia. The fact that lesions of the optic thalami cause paralysis of motion proves nothing regarding the real functional significance of these ganglia" (p. 239). Agreed; but then why does he not equally conclude that absence of paralysis of motion, when the corpora striata are destroyed, disproves the motor function he assigns to these ganglia, especially since direct stimulation of these ganglia does not produce motions?

Then, again, if sensorial excitations produce movements by playing upon the motor centres, why not adopt the view which regards all cerebral excitation as sensorial? The hypothesis of motor centres in the cortex would thus be resolved into the fact that particular sensations excite particular movements; and the localisation of spots on the cortex would be no more than analogous localisations on the skin—the sensation excited by tickling the sole of the foot, causing different movements from those caused by the same stimulus applied to the heel or instep.

Dr. Ferrier has explicitly declared that "there is no reason to suppose that one part of the brain is excitable and another not. The question is how the stimulation manifests itself" (p. 130). This is in accordance with what I have maintained, namely, that *neural processes* are uniform in character, the diversity of their results—sensation, motion, or secretion—depending on anatomical connections. *In itself*, a neural process is no more a sensation than it is a secretion. To determine a motor centre, therefore, we must look beyond the cortex, and detect its anatomical relations to the motor-apparatus. Do Dr. Ferrier's experiments prove that the area of the cortex, assigned by him as the motor area, has such anatomical relations to the motor apparatus, and the sensorial area such relations to the sensory organs, that we can speak of their activities as motor and sensorial *functions*? In other words, are the cortical areas to be regarded as playing the part of central functions or only of peripheral incitations?

He has argued his view with such force of fact and suggestion that I have little doubt of his carrying most readers with him; and because I dissent from his view I must occupy all my remaining space by endeavouring to weaken the effect of his argumentation. He considers that the indications naturally suggested by the observed facts of electrical stimulation are proved by the observed effects of disease and extirpation. Stimulation of particular spots is followed by definite movements; destruction of

those spots is followed by paralysis of those movements. The reader is led captive by what seems irresistible logic. The evidence seems decisive. How if the evidence should be illusory? That it is illusory may be shown, I think, under three heads:—

*First Head.*—The Italian physiologists Lussana and Le-moigne have specially called the attention of experimenters to the fact that very many recorded contradictions result from the not distinguishing between the first and second experimental periods, namely, the effects observable soon after the operation, and the effects which are observable when the disturbance has settled down, and the organism has recovered something like its normal state ("Fisiologia dei Centri Nervosi," 1871). The first period comprises what may be called the effects of Disturbance of Function; the second period the effects of Removal of Function. The distinction, so fruitfully introduced by Dr. Hughlings Jackson, of discharging lesions and destroying lesions falls under the same conception. I will only add that neither the effects of Disturbance nor the effects of Removal are to be taken as conclusive evidence that the function disturbed or removed is the function of the organ operated on; but that whenever a function persists, or reappears, after the destruction of an organ, this is absolutely conclusive against its being the function of that organ.

This premised, I must suggest that Dr. Ferrier's experiments cannot be considered as conclusive, because he was unable to keep the animals alive long enough to allow the effects of Disturbance to subside, so as to leave only the effects of Removal to be estimated. And this is the more to be emphasised because in some instances the animals did survive long enough to show some subsidence of the disturbance and *some reappearance of the lost functions*. Now the reappearance of a function after the destruction of an organ admits of but two interpretations—either the function was arrested as an effect of the disturbance, or its organ was destroyed, and *another* organ had vicariously taken its place. This second interpretation is much in vogue, and has received the name of the law of Substitution. The notion that a function can be driven from organ to organ, "like a sparrow driven from one branch to another," as Goltz picturesquely says, is surely raising Hypothesis to the *n*th power? Dr. Ferrier without adopting the first of the two interpretations, argues against the second with his usual force; replacing it by one which is physiologically more consistent, namely, that "there is no direct establishment of new centres in place of those which have been lost, but that those which remain may indirectly without assuming new functions make up for the loss, to some extent at least." In these cases "the path from impression to action is not as in the ordinary course of volition through the cortical motor centres to the corpus striatum, and thence downwards to the motor nuclei and motor nerves, but through the basal ganglia directly." This fails to meet the case when, for example, the function of vision on one side disappears after removal of its assigned cortical centre, and nevertheless reappears. We cannot get the ear to do the work of the eye; and if touch does indirectly make up for loss of sight, it is by a slow process of acquisition; whereas the animal recovers its lost *sight*, and that in the course of a few days.



But restricting the explanation to movements, is it not a relinquishment of the hypothesis of voluntary motor centres? Is it not an invocation of the hypothesis of peripheral incitation? Observe this, moreover: Dr. Ferrier restricts his explanation to the movements which have been automatically organised in the corpora striata. All actions not become automatic are impossible after removal of the cortical centres. "It may confidently be asserted," he says, "and perhaps it may one day be resolved by experiment, that any special tricks of movement which a dog may have learnt would be effectually paralysed by removal of the cortical centres." Well, since this was written, experiment *has* decided the point. By an ingenious method of washing away cerebral substance, Goltz has been able to greatly diminish the disastrous effects of operation, and thus preserved the animals for weeks, in the course of which he observed an *almost* complete restitution of the lost functions. One of the striking cases recorded by him (Pflüger's *Archiv*, xiii. 31) is that of a dog who had been taught to "give the paw" on command. When the surface of the left hemisphere had been washed away there was at first a complete destruction of the power to give the right paw; and the dog when urgently called upon to give it, looked wistful, and ended by stretching out the left paw. Had the dog died within six days after the operation this might have been cited as proof of the destruction of a voluntary centre; but the dog lived, and on the eighth day began to give the right paw when asked, and a month afterwards gave it as readily as before the operation.

*Second Head.*—Under this head we may consider the evidence adduced for the existence of definitely circumscribed areas, and definite spots within those areas. Dr. Ferrier's pages are very instructive on this point, but not, I think, competent to force his conclusion when they are confronted with Goltz's experiments, which show that the paralysis of sensation and motion cannot reasonably be assigned to the destruction of particular spots, because the paralysis is dependent solely on the *amount* of substance washed away, and not at all on the *localities*. Add to which the fact just insisted on, that the paralysis is temporary. Dr. Ferrier believes that his experiments prove the distinct localisation of motor centres. For example, he produces inflammation and suppuration in one place, and observes spasms followed by paralysis of motion in the whole of one side of the body. This is urged in proof of motion being affected without affection of sensation. On examination, however, it seems to me only to prove the effects of disturbance; and this the more decisively, because he admits than when instead of an *irritating suppuration* there is *extirpation* of the centre, the paralysis quickly disappears. "In these experiments," he adds, "the power of movement alone was destroyed, sensation remaining acute and unimpaired." This is very ambiguous. Sensation *elsewhere*—on the other side of the body—was unimpaired; but so was power of movement *there*. In the paralysed limbs there was no sensation.

Let us now turn to a sensational centre: and we will select that of Vision, because the experiments are here most striking. Destruction of the "angular gyrus" on one side causes blindness in the opposite eye. But this effect is temporary, and begins to subside the next day (p. 165). One would imagine that in presence of such

observations, the fact of blindness would be attributed to Disturbance, not Removal of Function; and the recovery of vision to the subsidence of the disturbance. Dr. Ferrier interprets the recovery as due to the compensatory action of the centre in the other hemisphere (p. 169). But this is to invoke the Law of Substitution (which he has successfully refuted), and leaves unexplained why the compensatory action did not manifest itself from the first. The experiments of Goltz seem to me conclusive as to the observed blindness being merely the effect of disturbance; not only does the vision gradually return, but is proved not to depend on the compensatory action of the intact centre, because it reappears even in an animal deprived of the other eye. That is to say a dog, with only one eye, had almost the whole of one hemisphere washed away, so that on the one side it had no optical apparatus, on the other no visual cortical centre—yet it showed unmistakable evidence of being able to see. Observe the dilemma: either there is a complete decussation of the optic nerves, so that each hemisphere is the sole centre for one of the eyes; or the decussation is partial, so that each hemisphere is a centre for both eyes. In the first case destruction of the one hemisphere should produce absolute and permanent blindness in one eye—and this is disproved by experiment. In the second case destruction of one hemisphere should produce partial blindness in both eyes—and this also is disproved by experiment. Or, finally, the visual centres are *not* in the hemispheres, so that destruction of the hemispheres is not destruction of vision—and *this* is what experiment proved.

*Third Head.*—I must be very brief on this point—namely, that very various effects ensue on excitation of one and the same spot. If we regard the cortex as a peripheral surface of excitation there is nothing mysterious in the various effects produced by reflexes from it—as from the skin; but if we regard it as a collection of distinct sensory and motor centres, there is great difficulty in reconciling the results of observation. For example, the so-called voluntary centres for movements of the limbs and tail are found by Rochefontaine to be centres of salivary secretion. In his memoir in the *Archives de Physiologie* (1876, No. 2, p. 169), the last-named experimenter sums up the results of his observations thus—that the hypothesis of cortical voluntary centres would lead to the conclusion that the same spot was the centre for voluntary movements in a limb, and involuntary contractions of the bladder and spleen, as well as dilatation of the pupil.

My space is exhausted, and I have not been able to do more than criticise the main topic of Dr. Ferrier's book—and this not with the fulness which its importance demands. But if I have shown grounds for regarding the hypothesis of voluntary centres in the cortex as at any rate far from *proved*, and in doing so have had to adopt an antagonistic attitude throughout my review, I should not be just to him, nor to my own feelings of gratitude, if I did not, in concluding, express a high sense of the value of his work, full as it is of suggestions, and rich in facts, which no counter-facts can set aside. It will long remain a storehouse to which all students must go for material. It may be the starting-point of a new anatomy of the brain.

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